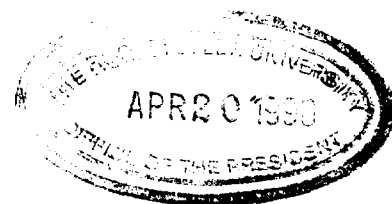


(X) ↓ Schomaker ~~at Paul~~
✓ 4/16/90



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April 16, 1990

Prof. Joshua Lederberg
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Your note came as a great surprise, paying more attention to my comment in Serafini than I had thought it ever deserved. I had seen it because Jürg Waser had been infuriated by Serafini's book and the Williams review in *Nature*, and had got me to work with him on our eventual letter (copy enclosed).

I have talked with Waser, Singer, and Itano, all of whom I happened to see in La Jolla recently, and to James Bonner, Norman Davidson, and Dick Marsh here, but not with Ray Owen or Walt Schroeder. All of these might remember something about the days of the 'synthetic' antibodies, either from having been on the scene or having a special interest. (It amazes me that aside from Pauling himself and J. B. Koepfli I may be the oldest local survivor in Chemistry of those days.)

The most important for your question, however, may be T. Harrison Davies, who had gone to work at Lederle after spending a couple of years here as a post-doc with Pauling and Coryell, and had been sent out to check on the mouse-protection report in, I believe, spring of 1943. (By summer of 1943 he had gone to the Met Lab, before going to Oak Ridge. After the war he spent a few years at the University of Chicago, then went to the Mellon Institute and eventually Carnegie-Mellon, and now, I believe, continues to live in Pittsburgh or nearby.) He spent a month or so and, as I think I recall quite clearly, told me at the end that he had got protection against Type-III pneumococcus infection with material prepared by the Campbell procedure, but had got the same degree of protection from γ -globulin that had not been so treated and had concluded that Dan must not have run adequate controls. I assumed that his report ended Lederle's interest in the matter and eased Harrison's decision to join Coryell in Chicago. You might find it interesting to get in touch with Harrison. (He may have been the "young man at Lederle" mentioned by Kay, p. 169.)

When I read your reference to the 'synthetic' antibodies paper as "besides being wrong, was a lousy experiment", I supposed that your reference was to mouse-protection as described in the 1942 *Science* and *J. Exp. Med.* papers. Now that I have read these papers, I have been surprised to see that they don't describe any mouse-protection experiments and that, indeed, only the first paper, in *Science*, refers to any prospect of doing such experiments. My impression at the time was that Pauling's greatest pleasure, yes euphoria, over the work had rested with the 'final' confirmation by the mouse protection, which I must have known about from seminars or talk in the lab, that he was disappointed by Davies's results but not discouraged, and that he fully expected that further work would confirm his hopes. As time went on, I believe I recall, we felt that Pauling's regard for Campbell and attitude toward him cooled. In any case, my fixed memory of this is determined by my understanding from Davies's report that there was reason enough to be disappointed or angry in the matter of controls. Now I have Jon Singer's word that in the years he worked in the group (1947-1950) Campbell was pretty much in Pauling's doghouse, all right.

Anyhow, I am not clear, even after reading Kay's thesis, on what was ever written about mouse-protection experiments. As I said, I was surprised not to find them described in the 1942 articles. As for any retraction by Pauling, we are all inclined to ask, "Did Pauling ever retract anything?" The brilliance of his ideas and insights are matched by his pride and confidence in being right, once he has taken a strong position; if it seems, on occasion, that he has been wrong, he still hopes that he is right after all (and believes - here I muse over the question as I have for many years - that it is worse to retract when he is really right than it is to refuse to give up when he is wrong but still has reason to hope that he is right; and if it should turn out that he *really* was wrong, then it's better to leave the sleeping dog lie).

My faith was strong still. I remember how Richard S. Farr in 1953-1954 told me about Jerne's ideas and had me meet Talmage. But I was lost in the belief in infinite variability and extent of specificity, and incapable of seeing the biological light. I evidently continued to believe in Pauling's theory of antibody synthesis, partly, I suppose, because he still seemed to - but here again I guess. Your note, "Linus was at the meeting, still defending his model", confirms my guess.

Jon Singer referred me to Pauling's introductory chapter in volume 15, 1986, of *Annual Reviews of Biophysics and Biophysical Chemistry*, mentioning that it might tell something of Pauling's recent views and then emphasizing his remarkable 1948 description of the still unknown structure of the gene. I enclose a copy of the relevant part, in case you've missed it. Pauling glides neatly past the 'synthetic' antibody story and then once more shows me how deficient I have been in keeping fully aware of his conceptual outpourings. I hadn't been aware of the Jesse Boot Lecture statement until I read it now, just as I hadn't been aware, until some twenty years later, of his 1948 assertion that an enzyme should be expected to be complementary not to its substrate but to the critical reaction state of the substrate. In any case, no retraction by LP here.

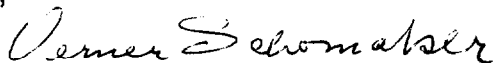
Dan Campbell was in poor health in his later years, I understand (I don't recall seeing him in my infrequent visits of the time), and died in 1974. (Serafini attributes to James Bonner a description of a scene of Pauling and Campbell that therefore couldn't have happened at the implied time - and probably wouldn't have ever happened.)

I don't know what came of Frank Dickey's experiment. After a fellowship in Europe, he may have returned here to Caltech for a year or so, but then got a job with Continental Oil Co., which, however, allowed him to work at home at the beach, rather than in Bartlesville. The job didn't last forever, but Frank has continued to live at the beach (Long Beach, Seal Beach, or thereabout).

Finally, I have to emphasize, if there is any need, that my remark to Serafini was made in respect to how the actual process of formation of antibodies had turned out to be altogether and most wonderfully different from anything that I could ever have imagined (or could have expected Pauling to imagine). I also probably indicated that Pauling had turned out to be wrong in having supposed that the structures of natural antibodies were not determined simply by their sequences. In that respect his theory, however reasonable it seemed to him or to us around him, didn't match his par. But I certainly didn't mean to suggest that the main body of his work on antibodies and antigens (as summarized, e.g., in the volume-15 *Annual Reviews* article) was "all pretty much on the wrong track". If it was, I am still blinded by its brilliance.

Yours,

VS:tex.



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